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Remarks to the First Conference on
the Economics of Remote Sensing Information Systems

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I am standing in for Brad Johnston, the Administrator for Applications in NASA. I am very pleased to be here, in spite of loosing my day off tomorrow, for two very personal reasons. First, my wife just might have gotten the idea it would be fun to stand in the sub-zero weather and watch Jimmy, and second, my professional duties have, over the past few years, been drawing me ever deeper into the world and abstractions of economics - deeper than I had planned many years ago when I gleefully threw Mr. Samuelson's Opus into the discard pile after the final exam.

I intend, before I am finished, to give you some of my reflections as a result of my enforced involvement - but first I would like to transmit some information to you about my world, namely where NASA is at the present time in regards to remote sensing - and where we think we are going.

First, let's put remote sensing of the LANDSAT type in its context in the overall NASA Earth Observations Program. When I rubbed the moondust out of my eyes at the end of the Apollo program - (let me note in passing that we managed to spend $25 billion, without a single cost benefit study) and accepted my present position as Director of Earth Observations Programs, the program had, and still has, three major focal points: Weather and Climate, Atmospheric Pollution, and Earth Resources. At that time, it was January 1973, there had been a total of 28 weather satellites, one earth resources satellite, then called ERTS, and a plan for an atmospheric oriented satellite - Nimbus G. It seemed to me that I had married a widow with three children - the oldest (Weather and Climate) had his Doctor's degree and was well respected in his

field of weather forecasting. Best of all, he had a job. The youngest (Atmospheric Pollution) had lots of promise, but also a way to go before he matured, or even before his education would be very expensive - the third (Earth Resources) was the flashiest - still in school, but already famous for the promise he was showing there. The only problem was in figuring out what he would do for a living when he graduated. He seemed to go in all directions at once - land use, world agricultural inventory, forestry, water shed management, etc., jack-of-all trades, master of none - well, here it is four years later, no job yet, he seems to have elected to stay in school and, let me tell you, its getting expensive to keep him there.

In the context of this metaphor, you are either his graduate examination committee or his employment counselors. Either way, you have, or will have, a hand in his future. Please hurry, he needs a job, and I need to get him off the family budget. I can sum up our present position as follows: LANDSAT's I and II have definitely proven that color interpretation of modest resolution multi-spectral data can do a wide variety of useful jobs - it can bring a new dimension of objectivity to photo interpretation. Through its capability for computer analysis, it provides an economical approach to repetitive analysis and inventory of natural resources over very large areas. In fact, I believe there is almost general agreement that the LANDSAT data has really introduced a totally new and revolutionary way of looking at our planet. "The small fragile planet" rhetoric of the Apollo days has produced a device which really can, for the first time, look at the total world. It can do it regularly - and, equally important, it can send its information simultaneously to those being looked at and to those who wish to integrate the whole world.

I think we "remote sensors" have a tendency to overlook the importance of the communicative side of our system. The fact that everyone in the world can get all, or any part, of the data we take is, in itself, a powerful tool drawing our tribal nations into a deeper sense of their common problems - it has been absolutely essential to the general international acceptance of LANDSAT's

"Open Skies" policy. So we have essentially proven a technology. We have two satellites flying and a third approaching its launch time - it will be ready by September of 1977 but may be delayed until February or March of 1978, if either LANDSAT I or II look as if they will be able to stand a few extra months. The new satellite, LANDSAT C, will have a new channel, sensitive to thermal radiation, and two black and white video cameras with forty-meter resolution.

Perhaps even more important to our objective of giving the users the data in a form and in a time period they can really use effectively, will be the revised all-digital ground system we will be using during this time period. It will provide all the data in either tape or picture form - very precisely registered to ground control points - and do this precisely with an average of one week transportation and processing time prior to availability to the user. This will certainly attack, and hopefully solve, the biggest and most universal user complaint LANDSAT has had - late delivery and no precision geometrical locationing.

I probably have not told most of you anything new yet - but I can now tell you that we are going forward in this budget with LANDSAT D - to be flown in the first quarter of 1981. This satellite will continue our experimental program for at least two, or, hopefully three more years. It will carry the already started Thematic Mapper - which is an enlarged MSS capable of thirty-meter resolution (versus the current MSS 80) - six bands (two of which are completely new, and four are modified MSS bands), greatly increased color sensitivity (four times the current MSS value). Our users have indicated to us that this capability will represent a reasonable limit to performance for the synoptic visible/IR sensor technology for at least the next decade. There will be instrument advances for special applications (indeed, the Thematic Mapper itself has room for a 7th channel) but it is hard to believe that any application which requires information at the national, state, or even at the county level will ever require repetitive looks at less than the 1/7 acre "pixel" of the Thematic Mapper. Beyond the Thematic Mapper lies the microwave world - with its sensitivity to moisture, its ability to pierce clouds and to develop shadows at many angles for geological interpretation.

LANDSAT D will have another advanced capability - it will transmit all of its data through the TDRSS system being developed for NASA's global communications network. This means no tape recorders to wear out - and a faster throughput of all data to GSFC, since the TDRSS system will send all the data to White Sands from whence it will be sent over Domsat to GSFC. We are planning a second generation data processing system to go along with our second generation flight instrument. This will automatically screen data for cloud cover or other defects - and will be capable of handling the ten-times-bigger Thematic Mapper data load as fast, or faster, than the present system.

As I noted at the start of my talk, I have found myself drawn inexorably into economic studies as NASA has tried to determine what it should be doing with its space-based technology. As an engineer in an engineering organization, I have a great deal of instinctive empathy with those who claim to be able to answer the question of how valuable the LANDSAT data will be in given situations. Especially when they come bearing mathematical models of total systems and talk of inputs and outputs, and all that good computer talk. However, while I really would like to believe that the world of economics was amenable to mathematical modeling, I find that when I look more closely at what you are saying, the following three situations seem to apply:

1. There is only one subject treated by an economic model in all the cost benefit studies to date - and that is crop inventory.
2. That even that case has received both positive and negative critical analysis by economists, some of whom are frankly skeptical of even the concept of capturing the complexities of international trade in a mathematical formulation, and even more skeptical of being able to determine the inputs to such a model realistically. But not only the inputs - the nature and meaning of the outputs seem to come under heated discussion also.

3. Where no model can be constructed we remain apparently in limbo - numbers are produced by expansion of case histories or aggregation of informed opinions - but the results are not disputed or accepted - they are just allowed to sit there.

Perhaps this conference will change all of this, but, if not, so what? Does anyone care?

I can't answer for your professional cares, but the answer in my case is simple - I care for two reasons - one, because there are people in the government decision process who are inclined to rely on the numbers produced by such studies, and two, because I have come to belive that our system designers need the insights which will be gained from economically oriented system studies. In the particular case I'm involved in - the benefit cost ratio of a LANDSAT based information system - I would sum up the situation as follows:

- Our studies, to date, have generally produced impressively positive ratios
- The studies have led to a strong feeling that a world agriculture inventory is almost the only economically valid use of LANDSAT.

I'm naturally pleased about the first situation - as my wife's Irish mother is wont to say, "it's better than a kick in the behind." But, I'm worried about the second. The second may even have some truth to it - but I am sure that the present feeling has been generated more by the tendency of decision makers to grasp at numbers, and especially at numbers that come from computers than by any really valid across the board comparison.

This is my major concern about the benefit-cost world. It appears that it, too, easily provides deciders with pat answers - the law of misplaced concreteness.

What are we to say about the benefits to the resource exploration industry where no one yet has ventured to construct a model - what about the contribution to the state and local governments - are they doomed to be ignored because their needs have yet to be expressed

in precise mathematical terms? Are we to charge the Department of Agriculture for the total LANDSAT bill, simply because one of their uses has been elegantly modelled and laboriously quantified by a computer? I would like to know how you feel about these side effects of benefit cost methodology. I don't know the answer, but I think we can say one thing. Decision makers have to simplify in order to be able to move in any direction at all - but they must be very careful about their simplifications. Also, I believe it is a fundamental responsibility of those who present "facts" in support of, or against, various courses of action to make very clear the percentage of "noise" in their facts, to point out the qualitative aspects of their assumptions - the limitations of their methodology. I appreciate Dr. Ray's comment on the youth of your technology.

I'm being idealistic - experts must, after all, be enthusiastic about their own techniques and products, or they wouldn't do the work, and worse, would not be called upon again for advice. Nobody loves a wishy-washy adviser.

Now let me talk about my own feelings about cost benefit methodology. Let me refer to the most complex mathematical modeling I have experienced previous to this: All during the Apollo program, NASA spent - I'm sure, multi-millions - on elaborate models purporting to give the probability of sending a man to the moon and returning him safely. These models included all the operating systems and analyzed each for the number of possible failure modes and the probability of each, and then integrated these failure modes and probabilities to arrive at a final chance of making it.

The modelers had their problems - yes, they could evaluate electronic circuits for redundancy or the lack of it, and they had some data on some electronic parts for failure probability - but what were they to say about the majority for which no such data were available - even more of a problem, what about mechanical systems, or worse, about humans in the loop? In terms of complexity of the total system and the intrinsic ability to quantify the inputs,

I would say they faced a far easier task than you do. And yet the Apollo program did not use the precisely quantified answers from these studies as the criteria for launching to the moon. The limitations noted above were well understood early in the game, and good design practice, extensive testing, system simulation, and experienced management-judgement led to the decision to go.

Why did they continue to improve and run these models throughout the program then? For two very good reasons: First, the process of creating them brought to light system interactions that the normal design processes, interface documents, etc., did not reveal. Second, the studies were very useful in a relative sense - re which system was the weak link - given more effort, what should we shape up, etc.

Perhaps I am simply a prisoner of my limited experience - but it seems to me the analogy is both realistic and useful. Thus, my appreciation of your work lies in the process more than the answers - in what it reveals about the required characteristics of the information we will be supplying, rather than the benefit mechanisms, amounts or recipiants. I have found that the interaction between the user/economist and the remote sensor/engineer required in any meaningful attempt to estimate benefit cost ratios has been very useful for the latter at least. We are gaining useful insights into what features of our sensing systems are the important ones - insights into how the total system should be designed and run - what new research should be pushed to best augment the abilities already on hand - the relative worth of precision versus timeliness, of precision versus data processing costs - yield versus area improvements.

Let me sum up: Technology is capable of a million solutions to a million problems. We must learn to choose the most important problems and the best solutions.

Benefit cost ratios are one way to sharpen our decisions -- but they still must be used with caution, with constant reminders of their limitations, and above all, with great doses of common sense.

And finally, we in NASA must continue to force this perhaps unnatural union of the economist and the engineer, if we are to ever hope to refine our own total system designs to the point where they will make a timely, useful input to the world of practical affairs.